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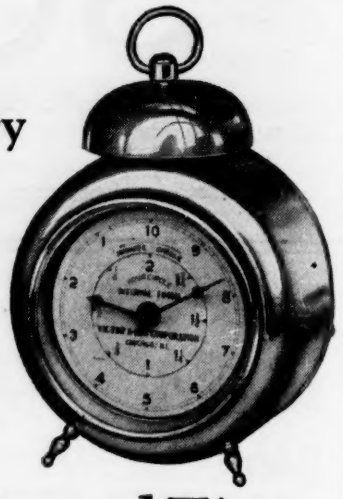

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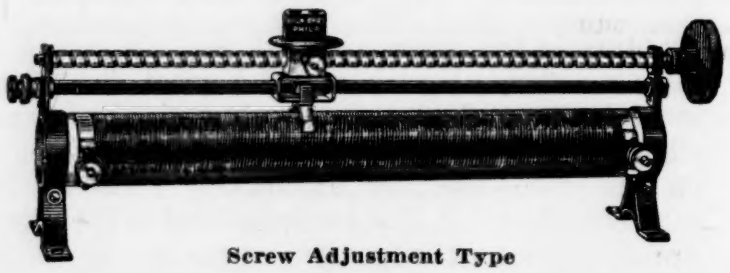

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ANCIENT AND MODERN ALCHEMY[1]

FIRST of all let me express my high appreciation of the honor of the invitation to come to Cornell as non-resident lecturer for the present term. When I received the friendly letter of Professor Dennis, my first thought was that it would give me the opportunity of staying for some time in the finest laboratory of chemistry now existing in the world, with which I was already acquainted from the description of the building that had been sent to me. It was with great pleasure that I accepted the invitation to join the well-known staff of the department and to avail myself of the privilege of presenting throughout the term some of my researches to a Cornell audience, and to continue my investigations in this building which offers such excellent facilities for experimental work.

It may be a matter of surprise to you that as the subject of my introductory lecture I have chosen alchemy, since that is not generally believed to belong to exact chemistry at all. Only a few decades ago Hermann Kopp, one of the best historians of chemistry, called the history of alchemy "the history of an error." If, however, it was an error, it was one of the most persistent of the false doctrines in the development of any science, and my reason for selecting alchemy as the topic of my address is to be found in the fact that the trend of modern chemistry is toward rather than away from the theories which were condemned by the official science of the last century, of which Kopp may be regarded as the representative.

Alchemy was in disrepute during really only a comparatively short space of time. For many centuries it was highly esteemed as the "sacred science" and no independent science of chemistry existed. It retained its dignity even when chemistry, as distinguished from alchemy, was being developed. It was never entirely abandoned, although, after chemistry had won a much higher position, the disfavor of scientists forced it for a time to hide in the obscurity of private laboratories and secret societies. In recent years it has again emerged into the full daylight of modern scientific theory and research. There is no doubt that much of alchemists' creed was "an error," but their idea that it must be possible to change one chemical element into another, as lead to silver or

[1] Introductory public lecture by Professor Fritz Paneth, of the University of Berlin, non-resident lecturer in chemistry at Cornell University.

silver to gold, has been strongly supported by the researches of our time.

But if the difficulty of observing an artificial transmutation of an element is so great that all the careful experiments of the chemists of a few years ago seemed definitely to disapprove such change, how is it to be explained that in former centuries the confidence in its correctness was so deep-rooted and that alchemy and the alchemists played such an important part in the life of that period? For the influence of the alchemists can hardly be overestimated. For example, official state papers of the sixteenth and seventeenth centuries make it clear that one of the important problems confronting a monarch or elector in Central Europe was to procure for his country an able alchemist who was expected to improve the financial status of the realm by transmuting base metals into valuable gold. It naturally followed that the alchemist was highly favored at court—so long as belief in his ability lasted. He was honored by the friendship of his sovereign and sometimes by elevation to nobility, and more than one of the crowned protectors of alchemy assisted personally in the experiments, so that he might convince himself of the correctness of the achievements of his alchemistic employee. The Emperor Rudolph II is reported to have himself worked with his alchemists, and a visitor to the Hradshin, the beautiful castle of Prague, the residence of the emperor, may even to-day see the five or six little houses, with disproportionately large fireplaces, which were built by Rudolph's command close to his own palace and which were used by his "gold-cooks." Rudolph appointed to a high position in his court Tycho de Brahe, who, although usually referred to in the history of science as an astronomer, was perhaps chosen by Rudolph because he was also of high repute as an alchemist. This is evidenced by the fact that the emperor provided him not only with an observatory but also with a laboratory for his chemical experiments.

In a more practical way Henry VI of England supported alchemistical experiments. To aid in the payment of the debts of the state he recommended to all noblemen, scholars and theologians the study of alchemy, and he conferred upon a company the privilege of making gold from base metals. This firm produced a metal (probably an alloy of copper and mercury) which had the appearance of gold and from this coins were stamped. History does not record whether King Henry believed that transmutation had actually been accomplished, but the careful Scotch were evidently skeptical, for the Scotch Parliament issued an order that this English "gold" should not be allowed to enter any of their ports or to cross their frontier.

The example given by the mightiest rulers of the time was imitated on a more modest scale by several of the smaller princes of Europe. Historical records tell us of one who tried to obtain a first-class alchemist from his neighbor, first by kindness and then by force; of another prince who loaned his alchemist to another court for a definite period, and of treaties between two states in which alchemists were regarded as mere chattels. Many of the rulers of that time were such firm believers in alchemistical doctrines that a lawyer of the period advocated making disbelief in these theories a *crimen laesae majestatis*. But although the lords of the realms generously supported the experiments of their alchemists, the financial returns never seemed to equal the disbursements. One repeatedly finds in the records that at the end of the research the sovereign lost his temper and that the alchemist, when hard pressed to show his product of manufactured gold, was usually well satisfied if he succeeded in escaping from the clutches of his former benefactor. If he failed to do so, he was severely punished and generally put to death. As showing the cruel humor of the times, it was a frequent joke to gild with tinsel the gibbet on which the alchemist was to meet his end.

We read of a great number of such executions and of innumerable failures of experiments. The successful transmutation of some cheap material into gold was very seldom reported, and in every case the transmutation, for some reason or other, could not be repeated: either the alchemist had disappeared or the stock of the "philosophers' stone," the miraculous powder which alone enabled him to accomplish "the great work," had been exhausted. The value of the gold that he claimed to have produced always amounted to a very small fraction of the money that had been spent upon him and his experiments.

How is it to be explained that in view of such constant and utter failures the belief in the possibility of the production of gold was not destroyed? *Three reasons may be offered.*

In the first place, there were some observations, which even from a critical standpoint seemed to prove the possibility of transmutation. The best chemical experts of that time were not greatly impressed by the results of the alchemists, for success invariably disappeared as soon as the experimenter was forced to permit a sharp control of his materials and apparatus. It was clearly recognized that in all such cases the element that the alchemist claimed to have manufactured was present in the material from the beginning, but was so well hidden, or so finely distributed, or in such chemical combination that the layman was unable to detect its presence. In the reputed art of gold-making they had no further proofs

of success than vague historical statements that on a certain occasion a powder, provided nearly always by an "unknown stranger," had exhibited the properties of the "philosophers' stone" and as evidence of the miraculous transmutation into gold accomplished by experiment a gold nugget of metal was shown. One may see at the present time in some of the European museums such gold products of the alchemists with detailed explanations of the manner of their production. But in no case the invaluable powder itself that had brought this about is to be found in any museum.

The early chemists were, however, firmly convinced that transmutation was possible and their conviction rested largely upon an experiment which clearly seemed to create copper and which could successfully be repeated at any time. This experiment consisted in immersing an iron vessel in the water of certain natural springs and allowing it to remain there for some hours. When it was removed its shape was unaltered but the vessel had apparently been changed to copper. I have seen such copper utensils with poetical inscriptions upon them to remind one of the mysterious origin of the metal. One reads thus:

Hart Eisen ich einst war.
Ein wasser rein und klar
Macht mich in wenig Stund'
Zu Kupfer in Herrngrund.[2]

Apparently a "water pure and clear" produced the transmutation. This water, however, was not as pure as it appeared to be, but contained traces of a copper salt, and when the iron vessel was immersed in the liquid the metallic copper, the "nobler" metal, was deposited upon the less noble metal, iron. The vessel was not, of course, changed as a whole into copper but simply received a thin coating of that metal. But the coating was real copper and showed all the details of the former iron surface.

It is not surprising that in those times when the small content of copper in the water could not be detected by chemical analysis, the phenomenon which I have just described was thought to be the transmutation of iron into copper; but on reading alchemistical treatises, one is nevertheless astonished to meet again and again this one example as the chief uncontested experimental argument for the whole doctrine of transmutation. This fact shows how scanty was the experimental evidence, in spite of the innumerable attempts which were not only made in the laboratories of emperors, kings and noblemen, but were also carried on by many commoners, and which never yielded gold in any appreciable quantity. From all this we may safely conclude that the reason for the firmness of the belief in the doctrine of alchemy was not the strength of the experimental evidence, but that it had quite a different basis, namely, psychological motives.

It is an old-known fact that men readily believe what they wish to believe. Modern psychology has deepened and extended the scientific knowledge of this fact by the discovery that such a wish is not less effective if it remains in our subconscious mind. Its presence may then be even more dangerous, since we are generally not aware to what degree it influences our thoughts and acts. Any strong wish may by "repression" obscure or extrude from our memory all that does not conform to it. Those of you who are acquainted with the work of the Vienna scientist, Sigmund Freud, will realize the importance of this field. I do not think that any psychologist will wonder that the judgment of those who lived in the days of the alchemists was misled by the wishes of such overpowering strength as played a rôle in connection with the idea of transmutation. It is not easy for us to-day to correctly estimate the power of this wish. It was not merely the hunger for gold. The habit of mind in those times found it quite natural to believe that a thing capable of changing all the metals into gold would also possess the faculty of curing every kind of illness. The all-embracing astrological conception of the world, to which the alchemical doctrine belonged, brought certain ideas into close relation by what we now consider to be only a weak analogy. In the case which we are discussing the analogy might be formulated—*the cure of metals and the cure of men;* for, using the terms of Albertus Magnus, gold was "the only metal free from any illness." The philosophers' stone, that mysterious powder for which every alchemist was searching, was believed to be able to change any other metal into gold by mere contact (they termed it "projection"), the stone being capable of transmuting many thousand times its own weight. It was further supposed to guarantee to its owner extremely long life, free from any shadow of illness. *Wealth* and *health,* these were the gifts of the philosophers' stone, and when you consider further that in the dark ages the ghosts of poverty and illness threatened the people in far more dreadful forms than in the present day, you will probably not think absurd the quotation which I recently saw in an alchemical treatise:

> Everybody must try to get two things, eternal bliss and earthly happiness: the former is granted by the Kingdom of God, which is taught by the theologian, while the latter is granted by the philosopher's stone of the alchemist.

Thus we can easily understand that under the influence of this vital struggle, people who were not

[2] Herrngrund is a small town in Hungary.

trained by profession in critical skepticism overestimated all which looked like a successful transmutation. But what about the scientists of that day? Surely they must have realized the weakness of the chemical proof of transmutation. But the most prominent men of science did not doubt its essential correctness, and this is to be explained by the third pillar of the alchemical creed, namely, that the doctrine of transmutation was in accordance with the universally accepted philosophical conceptions of the day.

We know that in the Middle Ages, and even in modern times, the system of Aristotle ruled the minds of men and that in science his theories were almost as firmly believed as were in religion the dogmas of the church. Now, according to Aristotle, all bodies are formed from a fundamental substance—the "primordial matter." This is pure matter without any form, and therefore not yet truly existent. If united with the properties *cold* and *wet,* it becomes the element of water. If it has the two properties *warm* and *wet,* it becomes the element air. To the element earth were ascribed the properties *cold* and *dry,* and to the element fire the properties *warm* and *dry.* The "four elements" in the Aristotelian sense are therefore nothing ultimate but only modifications of the primordial matter. By changing the properties, one can transmute one element into another. If in such a manner even these foundation stones of the whole sublunar world, bodies as different from each other as are fire and water, could be transmuted, no scholar trained in the philosophy of Aristotle could doubt that bodies so closely allied as are the metals could interchange their differentiating qualities. In manuscripts of the thirteenth century we read, as something almost self-evident, that silver, which in many respects is related to gold, can be changed into real gold more easily than can any other metal. It was quite the same conception which centuries later made the alchemists believe that by making copper white, which can be done by alloying it with other metals, they were on the way towards its conversion into silver, and that by giving to the copper a yellow color—think of the color of brass—they had achieved the first steps of the production of gold. Although, in other regards, such as the specific gravity of the product, the transmutation did not seem to have fully succeeded, it was nevertheless clear that the copper had been transmuted into something better. For, according to his theory of perception, Aristotle did not believe that, in a mixture of two metals, the components remained. This would have led him to the standpoint of his opponents, the atomistic philosophers, who discriminated between appearance and reality. Just as a drop of wine in a barrel of water disappears, according to Aristotle, not only for our senses but even in reality, so the element copper was supposed to be no longer present in brass.

From these brief references to the Aristotelian philosophy, it is easy to see that nothing could be more natural to a scholar educated in these ideas than the transmutation of metals. If the alchemists were not successful, it was surely the fault of those "sooty empirics," for the experimental workers were quite as highly disdained by the scientists of that time as by the philosophers of ancient Greece.

The authority of Aristotle in chemical theories was broken only when experimental research had won its independent position in science. The conviction then became general that metals could not be changed one into another in spite of the doctrine of the "primordial matter" and the transmutability of all bodies; and there gradually developed the belief that elements were not the four hypothetical bodies of Aristotle, nor the three "principles," salt, sulphur and mercury which some alchemists preferred, but were all such substances as could not experimentally be divided into simpler ones. It was urged that instead of fixing the number of elements *a priori* one should try to ascertain this number *a posteriori,* that is, by systematic experimental study. This new program was first developed by Joachim Jungius, a many-sided and ingenious scientist who lived at Hamburg in the first half of the seventeenth century. It is especially remarkable that Jungius further made a very clear attempt to substitute for the doctrine of Aristotle the atomic theory which later played such an important part in the development of modern chemistry. He, the author of a textbook on logic, was such a well-trained thinker that he did not fail to recognize that on the ground of the atomic theory the existence of isomerism and even stereoisomerism could be foreseen. Most of the papers of Jungius were distributed in the form of manuscripts and reached only a small number of sympathetic readers, but shortly after his death, there appeared a book of similar tendency in which the problems were presented in such attractive literary style that they appealed to the widest circles: that was "The Sceptical Chymist," by Robert Boyle. But even the immense influence of Boyle did not succeed in immediately subduing the elements of Aristotle. Only when Lavoisier, holding to the same definition that "a chemical element is a body which can not be resolved into simpler ones," found in the chemical balance a definite instrument for critically testing this belief, did the modern conception of a chemical element find general acceptance. Dalton completed the foundation of modern chemistry by the assumption

that every chemical element consisted of a special kind of indestructible atoms, and you know that this theory of the composition of matter dominated the tremendous development of chemistry during the nineteenth century.

This assumption of Dalton's, that there existed as many kinds of atoms with different qualities as there were chemical elements, so completely satisfied the theoretical needs of the chemist that the idea of a primordial matter fell into disrepute, but it was never completely forgotten. There were always some who felt that the existence of a great number of independent elements was unnatural. This feeling was strengthened by the discovery of the periodic system. The harmony which appeared in the ordering of the approximately seventy elements that were then known clearly showed that they did not consist of that number of perfectly independent chemical atoms; one had to return to the conception of something common to all these atoms or abandon all hope of explaining the interrelationships between the elements. Mendeleeff himself, it is true, was so convinced of the stability of the chemical elements that he violently criticized any hint that his periodic table supported the view that there existed a primordial substance. But it is a characteristic of important ideas that they very soon begin to live their own life in the mind of mankind, independent of the brain which first produced them. Just as the deeper understanding of Dalton's theory had to be developed against Dalton's opposition by Gay Lussac, Berzelius and others, in similar manner, quite contrary to the ideas of Mendeleeff, the periodic system has come to be regarded as strong evidence that the atoms of the elements are built up of smaller particles.

This view, derived by observation of the chemical behavior of the elements, was independently confirmed by physical researches. To explain the optical spectra of the elements, physicists were compelled to picture the atom not as a solid sphere but as a very complicated structure consisting of much smaller particles which moved inside the space formerly ascribed to the spherical atom. As to the nature of these particles the investigations of the physicists during recent years seem inevitably to lead to the conclusion that they are nothing else than the positive and negative building-stones of electricity.

One difficulty, however, seems immediately to arise. If the atomic structure of all the elements is so closely akin, how is the constancy of the elements to be explained? Why is it that they can not easily be transformed one into the other? Why did not the alchemists succeed in transmutation? The answer is that while *theoretically* we must concede the possibility of such transmutation as soon as we accept the view that the atoms are aggregations of smaller particles, yet *practically* the forces inside the atoms which hold these particles together may be so strong as to defeat attempts to effect a change.

Let me at this point call your attention to a historical fact which does not appear to be so widely known as it deserves. What I refer to is the theory of the structure of matter which was proposed by Robert Boyle. As experimental chemist, Boyle saw earlier than his contemporaries the necessity of assuming that there were many different chemical elements, but he considered this conception to be merely a necessary aid for the understanding of chemical reactions. As theorist and philosopher he adhered firmly to the old idea of a primary matter and he sought to explain in wholly modern fashion the qualitative differences of the elemental atoms by assuming different numbers and arrangements of the minute particles of the primary substance. That the elements remain unchanged in chemical reactions he considers to be due to the *relative* stability of these atoms. The "corpuscles of gold and mercury" are composed of minute particles of the primordial matter, but are nevertheless (I quote his words) "able to concur plentifully to the composition of several very different bodies without losing their own nature or texture, or having their cohesion violated by the divorce of their associated parts or ingredients." With an insight more remarkable even than that of his successors, Lavoisier, Dalton and Mendeleeff, he thus brought forward more than two hundred years ago a theory for bringing into accord the multiplicity of the chemical elements with the existence of a simple fundamental substance, a theory which modern science now bases upon an immeasurably richer assembly of facts. He lacked every possibility of experimentally demonstrating the composite nature of those substances which the strongest reagents of chemical analysis can not even to-day decompose. The theory of the complicated structure of the atoms of all elements was therefore with him only a philosophical postulate. Yet since he stated "that it will be scarce denyed that corpuscles of compounded nature may in all the wonted examples of chymists pass for elementary," it is self-evident that he always had in mind the possibility that a specially active agent might be discovered which would be able to pull the parts of the corpuscle asunder. He says, "There may be some agent found out so subtile and so powerful, at least in respect of those particular compounded corpuscles, as to be able to resolve them into those more simple ones, whereof they consist."

I have quoted Boyle's exact words because they seem to be almost prophetic of Rutherford's experi-

ments on the breakdown of the atom. Rutherford actually found this "agent so subtile and powerful." Yet previous to his success in experimentally disrupting an element, nature had given a much more definite indication than was furnished by the periodic system, spectra, etc., that the endeavors of the alchemists could perhaps be realized. This indication was found in the newly discovered radioactive bodies which furnished examples of chemical elements which *spontaneously* changed into other elements.

After Rutherford and Soddy had conceived the idea that the radiation from such substances resulted from the disruption of these radioactive bodies and the expulsion of fragments of the atoms with enormous velocity it seemed to them probable, for reasons which I will not take the time to present, that these fragments were nothing else than atoms of helium. If their supposition was correct, it appeared to follow that one should be able to detect the formation of the element helium from radio-active bodies and Ramsay and Soddy actually succeeded in experimentally proving that when radium breaks down helium is formed. This was the first instance in which one element, helium, was evolved from another element, radium. Both of them are real elements, for they can not be resolved by chemical means, and both show different chemical qualities and characteristic spectra. The experiment, therefore, established beyond a doubt the possibility of the transmutation of elements. But in a transmutation of this character it has been found that man has no power to influence it. The production of helium from radium takes place with absolute constancy, and no means at man's disposal, neither extremely high nor extremely low temperature, nor very high nor very low pressure, nor electric or chemical energy, can quicken or retard the rate of the transmutation. The radioactive substances appeared therefore to support fairly well the view that many held in the Middle Ages and even later. They thought that elements were formed in nature, that for example in the depths of the mountains bismuth changed in the progress of time into silver, and silver into gold, but that man could do nothing except to interrupt this ripening process at the proper moment and withdraw from the influence of the "mountain fire" the noble metal that had been produced. This withdrawal at the proper stage was deemed necessary, for they believed that with the further lapse of time the silver and the gold deteriorated and again reverted to base metal. (It may be pointed out in this connection that the alchemists claimed also to possess a substance, which as a negative philosophers' stone could destroy gold. This negative body was naturally much less highly prized than the real philosophers' stone.)

Several years after the experiments of Ramsay and Soddy, it developed that radioactive substances not merely show the elemental change which takes place of itself, but also have put in our hands the agent which can cause such a change at the will of the experimenter. It was again Rutherford who established this fact. His experiments, which are to be regarded as the first successful ones of alchemistical nature, are of such extraordinary significance that I will endeavor to give you an approximate idea of his technique so far as that is possible without the use of the special phraseology of radioactivity. A metal tube some decimeters in length is placed in a carefully darkened room and a radioactive preparation is put into one end of this tube. The substance sends through this tube rays which are themselves invisible to the naked eye. The preparation can be pushed into the tube to any desired distance. A glass plate in the further end of the tube is coated with zinc sulphide, a substance that possesses the property of glowing under the action of radium rays, just as the well-known Röntgen screen renders the X-rays visible. When the radium preparation stands at a distance of more than seven centimeters from the screen the latter does not glow, because this particular kind of radioactive rays travels only that distance. If a thin sheet of aluminum is placed between the radium preparation and the zinc sulphide screen it is to be expected that this barrier to the rays will extinguish the glowing of the screen at a shorter distance than seven centimeters. Experiments show that although the screen now becomes almost entirely dark there is still perceptible a very weak luminosity caused undoubtedly by the action of a small residue of the rays. But these residual rays behave in a very surprising manner. They reach the screen even when it is removed to a distance of ninety centimeters, although at this greater distance the glow is extremely faint. One can scarcely imagine a less striking experiment. The glow of the screen is indeed so feeble that only the trained eye can perceive it under the most favorable conditions. We must therefore marvel at the boldness of Rutherford, who ventured to draw revolutionary conclusions from this apparently negligible phenomenon. He argued that if behind the sheet of aluminum rays appear which reach further than those which fall upon the aluminum, these new rays must come from the aluminum itself; in other words, the aluminum atoms must be disrupted and send out particles from their inner structure, and the glow of the screen at a distance of ninety centimeters must be due to the impact of these fragments of the atoms. These particles can easily be caused to deviate from their paths by electrical or magnetic forces, and from the amount of this deviation Ruther-

ford concluded that they are particles of the size of the hydrogen atom. This means that the aluminum atom has been broken down and that hydrogen has been generated, although in so small an amount that it would take about a million years to obtain in this manner one cubic centimeter of the gas.

This experiment demonstrated actual atomic disintegration, and it has been found by Rutherford and by investigators in the Radium Institute of Vienna that not only aluminum, but also quite a number of other elements, such as sodium, potassium, phosphorus and chlorine, break down in this manner. This evidence seems clearly to indicate that hydrogen is the long-suspected primordial element. In spite of the unimpressive character of this experiment, the conclusions which one may draw from it are of far-reaching significance, and we have here a striking example of the fact that the importance of an investigation is to be judged not by its external brilliancy, but rather by the deductions that can logically be based upon it.

Thus we see that in a certain sense radium possesses the first and principal property ascribed to the philosophers' stone: it has the power of transmuting elements, although not of producing gold. And, oddly enough, even in respect to the second property which was ascribed to the philosophers' stone radium seems to have gotten something from its fabulous predecessor: it is a very valuable aid in the treatment of some severe diseases, although not a perfect remedy for every illness. So that to a certain degree the radium rays really produce the two very different effects of the philosophers' stone, transmutation and healing.

But in another direction this modern substitute of the philosophers' stone brought a severe disappointment. You remember the expectation of the alchemists that "by projection" the stone would transmute many thousand times its own weight. Unfortunately quite the contrary happens in such a case as the breakdown of aluminum by radium rays, for many hundred thousand atoms of the new stone must disintegrate before only one atom of hydrogen is formed.

The spontaneous disintegration of the radio-elements, and particularly Rutherford's success in artificially disrupting the atoms of some other elements, inspired investigations upon the artificial transmutation by other means. For the disruption of elements in the way shown by Rutherford is, strictly speaking, nothing but "induced radioactivity," if we may employ a term originally coined for another phenomenon, but which to-day is no longer used. With very large quantities of radioactive material, we can "induce" a hardly detectable activity in elements which are usually inactive. Of course it would be much more valuable to find a method of transmutation not limited to this very expensive and very slightly effective form of energy, and this thought revived the alchemistical experiments with renewed force. Scientific opinion had now reverted to the view of some critical scientists in the alchemical period, which is perhaps in the most concise form stated in a book by the polyhistoric Jesuit Athanasius Kircher: "Alchymia scibilis est, non tamen adhuc scitur." ("Alchemy is a science not yet known, but which may become known.")

Our return to this very view explains why to-day not only the daily papers but also scientific journals are ready to accept contributions to this theme. Even if we were to limit the present review to the articles appearing in scientific journals, the number of papers would be too large to allow of full discussion. But this need not cause us sorrow, because the publications of some of these modern authors remind us of the chemical ignorance and credulity of their ancient forerunners. Let me call to your attention, however, one or two publications of a higher type which caused wide discussion. You may have heard that just before the war, from the laboratory of Ramsay, Collie and Patterson announced that helium and neon were formed when an electric discharge was sent through hydrogen that was in a closed vessel. But shortly afterwards, Strutt, the present Lord Rayleigh, showed that this experiment could not be repeated if air, which always contains both helium and neon, was perfectly excluded. Nevertheless the assertion that they are thus formed was repeated a few months ago in the Proceedings of the Royal Society of London, but this experiment fails to convince one that Strutt's criticism is incorrect.

Many of you are doubtless familiar with the fact that last year a German chemist, Miethe, and a Japanese physicist, Nagaoka, independently asserted that by electric discharge gold may be formed from mercury. It is a special irony of fate that in this case alchemy reappeared in the old-known vestment of the artificial production of gold. Gold is particularly apt to cause this error because it may be present in various materials but so finely distributed as to escape detection by ordinary methods of analysis. But in various ways the gold may be concentrated to such an extent that it can now be detected without difficulty. Concentration was, therefore, in all the ages very often misunderstood as production. In the experiments of Miethe and of Nagaoka quite the same happened. To-day extremely small quantities of gold can be recognized and the scarcely visible beads of gold which Miethe could identify under the microscope would have entirely escaped the observation of

the alchemists of the seventeenth century. But if in the final product we are satisfied with such small quantities of gold, this necessitates the employment of much more delicate methods of ascertaining that the original material is perfectly free from gold. Hence the difficulties and the danger of error have been about the same in all centuries. That this modern "transmutation," like that of old, amounted only to a concentration of gold and not to its genesis, has been proved by Tiede, Riesenfeld, Haber and their coworkers in Germany and by Sheldon, Estey and Harkins in this country.

Permit me, before closing, to mention the latest experiments upon the transmutation of elements. These are described in an article by my assistant, Dr. Peters, and myself in the last number of the *Journal* of the German Chemical Society, under the title, "The Transmutation of Hydrogen into Helium." I am compelled to confess to you that I was not only interested in the literature of alchemy, but I even ventured to take up experiments along this line. For some reasons, particularly because of the sharpness with which the smallest quantities of helium can be recognized, I thought it promising to test the transmutability of hydrogen into helium and the paper mentioned above contains the results of experiments which we have carried on during the last three years. To discuss the details of these experiments would carry me beyond the limits of an address upon the general subject of alchemy. This transmutation we accomplished not by electric discharges, as tried by others, but by simple catalytic action. It is my hope that they soon may be substantiated by others. I hope further to be able here at Cornell to show that these experiments work as well in the new world as in the old.

If at the end of this lecture we try to compare ancient and modern alchemy, we can not fail to realize that modern alchemy, at least in so far as serious workers are concerned, is a matter of theoretical knowledge, not of practical utility. This distinguishes it from the attempts of the many thousands who carried on alchemistical work in the earlier centuries with the purpose of getting rich as quickly as possible. But let us not be unjust toward those in earlier times who were interested in alchemical experiments in the same manner as in the other theoretical problems of natural history. As early as in the thirteenth century the Franciscan monk, Roger Bacon, distinguished between *alchimia practica* and *alchimia speculativa*. Later we note that Newton, Leibnitz, Tycho de Brahe and Goethe, to mention the names of only a few of the greatest, showed in some periods or during their whole life a distinct interest for this much-disputed science, and their interests are surely to be classified under *alchimia speculativa*. But such men were exceptions in the olden time, as great exceptions as are to-day, we hope, the men who expect pecuniary profit from such experiments. Of course, we will not deny the possibility that sometime in the future practical profit may result. We should not forget that, for example, wireless telegraphy, with its enormous importance for most of the branches of modern civilization, has its origin in the purely scientific and practically useless experiments of Heinrich Hertz. It is quite impossible to prophesy that alchemy will never have practical importance, but any one who to-day would enter this field with the idea of deriving pecuniary advantage from it will surely be disappointed.

These considerations remind us of the three pillars on which old alchemy rested. We see that two of them rotted away. The pretended transmutation of visible quantities was recognized as a mistake, and the longing for riches and health, which by no means has disappeared in our times, can be more quickly and safely realized in other ways than by experiments in transmutation. But the third, the pillar of natural philosophy, as we may concisely call it, still stands. The tendency which induced the old Greek philosophers to search for a uniform primordial matter behind the complexity of phenomena is still at work in the considerations of modern natural philosophers and, curiously enough, this struggle at last shows success. The ancient hypothesis that a uniform primordial matter might exist has been substantiated by modern knowledge, at first theoretically and later experimentally.

Here we arrive at the gate of a new problem. Its discussion belongs to the field of philosophy rather than of chemistry and consequently I will only briefly touch upon it in this lecture. How is it possible that philosophers, so often despised by naturalists, could predict a scientific result centuries in advance? Different answers are possible. One could regard it as a mere casual coincidence of some old doctrines with the present state of our experimental knowledge. In this connection it is worth while to remind oneself that only one and a half centuries ago the well-known French chemist, Macquer, pointed out the important coincidence that the four elements of Aristotle were substantiated by the results of the most modern chemical analyses. But I am not willing to concede that the conclusions of to-day are based on such an unsteady foundation as that so-called "experimental proof for the Aristotelian elements." Secondly, following the school of the New-Kantian Philosophers, one could regard our problem as an example that the laws of nature are in the deepest sense created by the human mind and do not exist in an objective

world. It is quite impossible on this occasion to discuss this philosophical tenet which has so large a number of followers, but I think you will agree with me that careful scrutiny of the very history of alchemy shows that it does not support this doctrine. Astrology, to which alchemy belonged, attempted to prescribe to nature the laws which it should follow, laws which, developed in detail, were expected to govern the mutual relationships between metals and planets, between microcosmos and macrocosmos, etc. But nature did not consent to be governed by these laws, and generations of thinkers could not impress them upon her. But when the chemist, following the example of Boyle, abandoned the idea of establishing laws *a priori* and entered upon the experimental study of natural phenomena, the way was opened for the successful development of chemistry. And thus we see that in contrast with the philosophical standpoint mentioned above, the history of chemistry can only strengthen our belief that the laws of nature are independent of the human mind in their existence, not in their conception, a belief shared since antiquity by some schools of philosophy.

We must therefore regard the return of modern science to the old ideas of the Greek philosophers as a sign that they have correctly realized a principle which was formulated in different ways during the development of science and which Kepler worded as follows—"Nature likes simplicity." This term "simplicity" does not mean that nature always acts in the simplest manner that can be imagined. Kepler's own example serves to demonstrate this. The astronomical system which he developed and in which it was assumed that the planets revolved in elliptic orbits is infinitely more simple than the old one which it replaced, but elliptic orbits are not the most simple that we can imagine: circles would be still simpler, but the planets do not rotate in circles. The same considerations hold as regards the idea of primordial matter. Strictly speaking, we do not now think that there is one primordial substance but rather that there are two. The atom of hydrogen consists of both positively and negatively charged particles, protons and electrons, and it now does not seem probable that we will reach a simpler view of this structure. But the general tendency of the Greek philosophers, especially of those belonging to the Atomistic School, to remove complexity as far as possible and to assume quantitative differences instead of the qualitative ones we must regard as a sound principle of natural philosophy throughout the ages. Therefore, if modern and ancient alchemy are very closely in agreement as to the existence of a primordial matter, this should be regarded not as a mere accident nor as an impress of human ideas upon nature, but as a distinct evidence that from earliest times eminent thinkers have rightly conceived the unity in the multiplicity of things. The greatest significance of modern alchemy is that it has enormously strengthened this early conception and has furnished convincing proof of the unity of the material universe.

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A SUGGESTED COURSE IN PLANT PHYSIOLOGY

LEPESCHKIN,[1] in his recent book "Pflanzenphysiologie," states that plant physiology can only further develop hand in hand with physics and chemistry. Van't Hoff[2] also recognized the dependence of one branch of science upon another. He represented this relation of the sciences by arranging them in order of their increasing complexity—mathematics, physics, chemistry and biology. The rapid development in the last few years of new and more exact methods in physics and chemistry makes this relation appear much more important than it did a generation ago. Thus a student who endeavors to further the development of plant physiology must be prepared in the fundamental principles of at least three branches of science. Under the system of prescribed work common in most American universities, a student has little opportunity to get more than the required courses for a major in one department and a minor in another. The prerequisite for plant physiology as found in all university catalogues is elementary botany. An examination of the catalogues of thirty-three of the larger universities and colleges offering courses in plant physiology shows the following:

No prerequisites in physics or chemistry	14
Elementary physics or chemistry desirable or required	19
Required chemistry or physics beyond an elementary course	0

When one considers the complexity of physiological problems, it is evident that the preparation of students for progressive work in plant physiology is quite inadequate. Experience with advanced students of botany extending over a period of six or seven years has shown that only the exceptional ones have the ability to do experiments accurately which involve simple physical and chemical methods. If we admit the truth of Lepeschkin's contention, it is difficult to foresee a rapid development of plant physiology as a

[1] Lepeschkin, W., "Pflanzenphysiologie," Vorwort, 1925.

[2] Van't Hoff, J. H., *Zeit. für Anorganische Chemie*, 18, p. 1, 1898.

branch of the natural sciences, until plant physiologists are properly trained.

The lack of adequate preparation of those doing research is reflected by the lack of data and information determined by modern physical and chemical methods in recent text-books on the subject; also by repeated appearances of data and information which are either incorrect or misleading, without proper criticism. This is particularly true for the subject of photosynthesis. A few specific examples will serve to support the above statement. Books on plant physiology by Palladin,[3] and Lepeschkin,[4] give the results of experiments to show the relation of the rate of photosynthesis to the wave-length of light. These experiments have been determined by means of prismatic spectra. The student is naturally led to believe that the relation in such a spectrum is the same as would occur in the incident light. This, doubtless, is not true because the intensity per unit area for the different regions in the spectrum is affected by the index of refraction, which is a function of the wave-length. We realize that information is often given for its historical value, as, for example, Engelmann's classical experiments, but in such cases criticisms are essential; or it should be accompanied by corrected information, according to the latest knowledge. Such criticisms and information have appeared only recently in Benecke and Jost, "Pflanzenphysiologie."[5] A similar interesting illustration is given by Palladin,[6] in which the correlation is shown between the rate of photosynthesis as determined in a prismatic spectrum and the energy distribution in a normal spectrum which was determined with a diffraction grating. This relation is purely accidental as far as the data are concerned, because we know that the maximum in the normal solar spectrum is in the red end in the morning and evening and in the blue end at noon, especially in the summer time. This is evident from the change of color of the sun in the morning and evening as compared with its color at noon, also from astronomical data.[7] Thus it would be quite possible to get a curve of the energy distribution of sunlight which would show a correlation with almost any curve showing the rate of photosynthesis. If the former curve had been corrected by calculation to what it would have been in a normal spectrum, there would then have been no correlation in the two curves. Further, this correction can not be made by dividing the rate of photosynthesis by the width of the absorption band.[6]

Again, Figure 35 in Lepeschkin's "Pflanzenphysiologie," showing the path of light in a prism is inaccurate in that the lines do not show the breaking up of the incident light into its component parts, and the points of focus where only a pure spectrum can at all be obtained, as is represented in elementary text-books of physics.[8] This is an important point which has been overlooked by many text-book writers on plant physiology, and hence inaccurate information is passed on to the students. It is very evident that many of the data on this subject have been determined with no attention paid to the purity of the spectrum used.

The lack of essential critical suggestions is also common in chemical data. It is well known that chemical analyses of plant material vary considerably with the method used, yet, in text-books, data are given to illustrate a point with no suggestion as to the method used or its accuracy. On account of such information as this, many students are slow to free their minds of the impression that if a fact is printed it is necessarily true or exact. Likewise, they are also slow in acquiring the ability to analyze critically experimental evidence and to form their opinions with impartial judgment, which is so essential in scientific work. Many investigators who are doing plant physiological work have been systematically trained in chemistry and have acquired their botanical training when the occasion demanded, or they have been systematically trained in botany and acquired their chemistry and physics. Under the present system of required schedules common in most American universities, it is often necessary for students of botany doing research work in plant physiology to stop their investigation to acquire the technique of the physical and chemical methods required for their problems. We have observed numerous instances where only the routine manipulations of ordinary physical and chemical methods applied to plant physiological problems have been learned, with no understanding of the principles underlying the use of these methods, the experimental errors, the degree of accuracy, and so forth. When students do not thoroughly know the principles upon which the methods depend, it is nearly impossible for them to adapt the methods to new conditions or to offer constructive criticisms.

As a remedy to these conditions, many have advocated cooperative research by individuals prepared

[3] Palladin, V. I., "Plant Physiology," Eng. Trans., p. 23, 1923.

[4] Lepeschkin, W., "Pflanzenphysiologie," p. 116, 1925.

[5] Benecke, W., and Jost, L., "Pflanzenphysiologie," 1924.

[6] Palladin, V. I., "Plant Physiology," p. 24, Fig. 15, 1923.

[7] Abbot, C. G., and Fowle, F. E., Ann. Astr. Obs. Smithson. Inst. p. 104, 1908.

[8] Stewart, O. M., "Physics," pp. 665–667, Ginn and Co., 1924.

in different branches of science. Such cooperation has often resulted in many discoveries which might otherwise have been impossible. It does not, however, remove the lack of appreciation of viewpoints held by investigators trained in different branches of science. This appreciation is obtained by a systematic and intelligent course of study. Also, such cooperation is often impossible, particularly for research students in different departments. Too often much time is wasted by the individual in pointing out the mistakes and the shortcomings of the other, without helpful and constructive criticism.

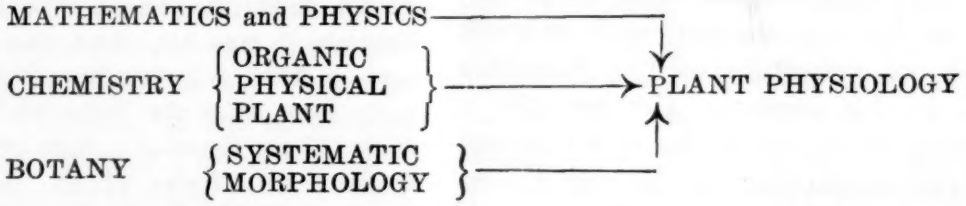
This lack of appreciation was recently called to our attention in a striking manner. An inquiry was made by a botanist about an astronomical publication on solar radiation. The astronomer replied that there was nothing in the article that could be appreciated or be of any value to a botanist. In contrast to this lack of appreciation for the necessity of cooperation we may quote a prominent scientist on the problem of photosynthesis:[9]

> . . . the chemical reactions constituting the photosynthetic processes are of a highly complex and intricate nature, sufficient investigation has been done to justify the conclusion that the problem is amendable to physico-chemical treatment. However no single academic division of science, such as botany, chemistry or physics is of itself sufficiently rich in concepts and methods to attack the problem adequately. . . . In view of the present academic divisions of the sciences and the variety of special training which is requisite for such undertaking, cooperative effort offers the only rational method of advance.

Again, we are told that an eminent chemist once said that the progress of our knowledge of photosynthesis has been retarded more than a hundred years by being in the hands of botanists. No statement was made as to the condition of our knowledge if it had been in the hands of chemists. It is not difficult to cite many examples in which valid criticism can be offered of chemical research on plant material because of lack of appreciation of the importance of plant structure and physiological processes.

We believe that the plant physiologist is largely a product of his training, and if his training does not prepare him to do research work which will develop his field of science, it is not, as a rule, satisfactorily acquired when the occasion demands. We also believe that the demand for plant physiologists will greatly increase when they are better prepared to handle the problems to be solved. As a step toward making it possible for those interested in research work in plant physiology to obtain the necessary fundamental training in the minimum amount of time, we suggest that a course in plant physiology be offered by the larger universities.

The following scheme may serve to show the relation of the fundamental sciences to plant physiology and suggest the necessary subjects.



It is understood, of course, that these subjects follow the usual prerequisites. A detailed outline of a course will depend upon conditions and individual interests.

It is quite probable that this course will not satisfy the requirements of any one department in most universities having a major and minor system. Thus it is necessary to have *departmental cooperation,* especially between botany, chemistry and physics. We believe that this *departmental cooperation* will greatly stimulate interest and facilitate *individual cooperation* which is necessary in the more technical and highly specialized branches of research in plant physiology.

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[9] Spoehr, H. A., *Jr. Ind. and Eng. Chem.* 14, p. 1145, 1922.

SCIENTIFIC EVENTS

THE BRITISH NATIONAL INSTITUTE OF POULTRY HUSBANDRY

THE selection of the Harper Adams Agricultural College as the site for the National Institute of Poultry Husbandry—the nucleus of the Ministry of Agriculture scheme for the development of the poultry industry—was influenced to a great extent by the value of the work already accomplished for poultry keeping at that institution.

The London *Times* reports that the equipment of the institute, which the Duke of York will formally open on November 3, is practically complete apart from the administrative building. For practical purposes the experimental station is already in operation, and during the present season important work has

been begun in breeding, hatching and rearing. Unfortunately, Professor Willard Thompson, the director, who is mainly responsible for the planning of the equipment, is returning to America next month, and his successor has not yet been appointed, though it is unlikely that the change will affect the continuity of the policy.

The institute will be devoted to the practical testing of scientific theories and knowledge. Information derived from purely scientific research at Cambridge and elsewhere will be submitted to investigation under practical conditions, and the results will be carefully analyzed and tabulated. Methods of housing will enter largely into the scheme, and criticism of some of the designs that have been adopted would be premature in view of the fact that the purpose is to break new ground and not merely to confirm principles already adopted in this country. For example, a square laying house, 20 ft. by 20 ft., of a type not uncommon in America, has been set up with a view to testing such accommodation for farm flocks, and it is claimed that a square building entails less structural cost than a long one. It remains to be seen, however, whether the window-space is sufficient and the interior adequately lighted to afford proper scratching quarters for a large flock of hens during an English winter.

The institute provides for a practical test of all systems of housing and rearing, while special interest attaches to its investigation of food problems. Some striking results have already been obtained by the use of milk in rearing chickens, and also for providing a supplementary food for laying stock in the place of meat and fish meal; and it would appear that where there is an economical supply of milk, such as is obtained at the institute from goats which subsist largely upon the herbage in the stock runs, a substantial gain will result. A portion of the experiment farm has been set aside for waterfowl, and pigeons and rabbits are also included in the scheme, which is likely to be of considerable benefit to the small farmer.

MORTALITY IN GERMANY

A CORRESPONDENT of the *Journal* of the American Medical Association writes as follows in regard to the effort of Germany to attain the lowest mortality rate in Europe:

For a century, Norway had the lowest mortality in Europe. As early as the middle of the eighteenth century, infant mortality in Norway was as low as it is to-day in the German *reich* because of the widespread application of advanced principles of social welfare. Hence, the chances of attaining old age in Norway were better than in other countries. In recent years, however, Germany, weakened by war and by scarcity of food, is among the nations in which mortality conditions are most favorable. Oberregierungs-Rat Roesle, in an article in the *Deutsche medizinische Wochenschrift*, points out that Germany does not lack much of holding the European low mortality record. As early as 1920, the mortality of Germany dropped to 15.1 per thousand of population, thus attaining very nearly the prewar minimum. In 1924 and 1925, the rate reached the low figures of 12.12 and 11.9, respectively. The contest with Norway, among the European nations, began in the first decade of this century, when the mortality rate of Denmark threatened to wrest the record from Norway. In 1912, the Netherlands, a new competitor, appeared. The mortality rate of the Netherlands for the years 1923, 1924 and 1925 nearly equaled the world record, which New Zealand, with a rate of 8.3, holds at present. In 1923, the rate for the Netherlands was 9.9, which was as low as that of Australia in 1924, and then dropped to 9.2. The lowest mortality rates after the Netherlands are recorded in Denmark (11 per thousand), Norway (11.1), Sweden (11.4), England and Wales (11.6), Switzerland (11.8), and Germany (11.9). In almost all those countries, however, the birth rate threatens to fall lower than the lowest birth rate record, heretofore held by France. Whereas in France the declining birth rate has, since 1922, been brought to a standstill, as was the case before the war, in all the countries with strikingly low mortality rates the birth rate has fallen markedly. Two countries, England and Sweden, have attained the minimum for France; Switzerland reached it in 1923. Also Germany's birth rate is only slightly higher than that of France. In 1924, the birth rate for Germany (living births) was 20.5 per thousand of population; the rate for France was 19.2, for England, 18.3, and for Sweden, 18.1. The low birth rate, therefore, which results in a low infant mortality rate, influences favorably the general mortality rate. Low mortality rates in the countries in which they obtain go hand in hand with low infant mortality rates. Since the war, a new record for infant mortality has been established. Previously, Norway, with a figure of 6.4 per hundred living births, had the lowest infant mortality, but Sweden for 1923 announced a rate of 5.6 and the Netherlands a rate of 5.7. Switzerland, England and Denmark reached the lowest infant mortality rates of record for their respective countries: 6, 6.9 and 7.7. Germany, whose infant mortality rate in 1921, 1922 and 1923 hovered around 13, in 1924 recorded a rate of 10.8, thus gaining a position among the countries with the lowest infant mortality rates. The most important aspect of the situation is the excess of births over deaths, which constitutes an expression of the natural increase in population. In increase oi population the Netherlands surpasses all other European countries, with an excess of births over deaths of 10 per thousand of population, whereas the corresponding excess in Germany, England, Sweden, Switzerland, France, Belgium and Austria since 1922 has always been below that figure. Also the tuberculosis mortality rate for Germany has fallen considerably, and bids fair soon to overtake the countries with the best

records in that regard; namely, Denmark, England, the Netherlands and Belgium. According to Roesle, not only the general mortality rate but also the infant and the tuberculosis mortality rates have thus taken a very different course, during recent years, in Germany than was prophesied in many quarters.

FALL EXCURSION OF THE NEW ENGLAND INTERCOLLEGIATE GEOLOGISTS

THE twenty-second annual excursion of the New England Intercollegiate Geologists was held in the vicinity of New Haven, October 8 and 9. Dr. C. R. Longwell, of Yale University, made the arrangements for the excursion. Friday afternoon points of interest about New Haven were visited. The intrusive dikes and sheets at West Rock, Pine Rock and East Rock were of special interest because of the classic studies made by Professor J. D. Dana. They also served to stimulate a discussion of the advance of geologic thought since the death of Professor Dana.

On Saturday the group studied the Anterior Trap sheet north of East Haven, the major fault between the Triassic basin and the rocks of the Eastern Highland of Connecticut south of Beacon Hill, the replacement of the Middletown series by granite near Haycock Point, the geology of the new reservoir of the New Haven Water Company near North Branford, the "fanglomerates" at the base of Quonnipaug Mountain in North Guilford and an explosive volcanic vent south of Durham. The day's work centered about the question concerning the method of formation of the Triassic depression. It was generally agreed that the theory illustrated by Barrell's diagrams was correct.

The attendance was record breaking. The institutions represented were Brown (8), Choate School (1), Clark University (4), Columbia University (1), Colby (2), Hartford High School (3), Harvard (8), Mount Holyoke (4), Norwalk High School (1), Rhode Island Teachers College (2), Smith College (4), Trinity College (3), Tufts (1), University of Vermont (1), Wesleyan (4), Williams (1), Yale (23). The total attendance was therefore seventy-one, over twenty more than were ever present before.

WILBUR G. FOYE,
Secretary

APPOINTMENTS AT STANFORD UNIVERSITY

STANFORD UNIVERSITY, following out its plan of development, has made a number of new appointments to its faculty this year.

Dr. Arthur L. Bloomfield, an authority on diseases of the throat and respiratory tract, has gone from the Johns Hopkins University to become head of the department of medicine in the Stanford Medical School, succeeding the late Dr. A. W. Hewlett. In the Medical School, also, Dr. Emile Holman has become head of the department of surgery as successor to Dr. Stanley Stillman, who has retired. Dr. Philip E. Smith, for fourteen years connected with the department of anatomy at the University of California, has become associate professor in the same department at Stanford. Dr. Frederick Leet Reichert is taking the place of Dr. Edward B. Towne as associate professor of surgery at the Stanford Hospital.

The new head of the department of mechanical engineering, Arthur B. Domonoske, formerly of the University of California, has taken up his duties, succeeding Professor Everett Parker Lesley, who is devoting all his time to his work as one of the division heads in the newly instituted Daniel Guggenheim Laboratory of Aeronautics. Similarly, in the absence of Professor Eliot Blackwelder, of the department of geology, Dr. Paul MacClintock, of the department of geology at the University of Chicago and dean in the college of arts, literature and science there, is at Stanford during the present quarter.

With the addition this year of Dr. James W. McBain to the faculty of the department of chemistry one of the most important laboratories for research in the field of colloid chemistry will be established. Dr. McBain, now Leverhulme professor of physical science at Bristol, holding a chair endowed for him individually by Lord Leverhulme, a distinguished leader in his specialty of colloids, will take up his work in January, being on leave for the first quarter of this year. With him will come also his two chief aids at Bristol, Dr. Mary Evelyn Laing, as research associate, and Miss Mabel Harriett Norris, research assistant. They will join the Stanford faculty, enabling Dr. McBain to continue without interruption the investigations on which he has been engaged and to proceed with new work.

SCIENTIFIC NOTES AND NEWS

THE Nobel prize for medicine has been awarded to Professor Johannes Fibiger, of the University of Copenhagen, for his work on cancer.

DR. W. D. COOLIDGE, assistant director of the General Electric Company's research laboratory at Schenectady, N. Y., gave, at the Franklin Institute on October 19, the first public demonstration of his new cathode ray tube, a preliminary account of which was given in SCIENCE for November 13, 1925. At the same time the institute presented Dr. Coolidge with their Howard N. Potts gold medal, given for "distinguished work in science or the arts."

THE Perkin medal for 1927 has been awarded to Dr. John Teeple, treasurer of the American Chemical Society, for "significant scientific, technical and ad-

ministrative achievements, particularly the economic development of an American potassium industry at Searles Lake, Calif." The medal will be formally presented by Dr. William H. Nichols, senior past president of the American Society of Chemical Industry, at a meeting in Rumford Hall, New York City, on January 14 next. The committee on award consists of representatives of the American section of the British Society of Chemical Industry, the American Chemical Society, the American Electrochemical Society, the American Institute of Chemical Engineers and the American section of the Société de Chimie Industrielle.

As part of the dedication exercises of the new Samuel Mather Science Hall, Kenyon College conferred the honorary degree of doctor of science on Dr. W. E. Forsythe, director of the Nela Research Laboratories at Cleveland, and on Professor J. C. W. Frazer, of the Johns Hopkins University. The degree of doctor of laws was conferred upon Professor M. I. Pupin, of Columbia University; Professor E. M. East, of Harvard University, and Professor C. K. Leith, of the University of Wisconsin. The principal addresses were delivered by Dr. Leith and Dr. Pupin.

THE Boston Surgical Society will award the Bigelow medal this year to Dr. Rudolph Matas, professor of surgery at Tulane University. The presentation exercises will take place on November 1. The introduction will be made by Dr. C. A. Porter, president of the society; the address will be given by Dr. Matas on "The Spirit and Art of Surgery." The presentation of the medal will be by Dr. Harvey Cushing.

DR. ALEXANDER C. HUMPHREYS, for the past twenty years president of the Stevens Institute of Technology, has presented his resignation to take effect next June.

PROFESSOR ELIZABETH FISHER, head of the department of geology and geography in Wellesley College since 1908, has retired under the provisions of the Carnegie Foundation as professor emeritus.

DR. LOUIS F. JERMAIN, for fourteen years dean of the Marquette University School of Medicine, Milwaukee, tendered his resignation at a recent meeting of the board of trustees and was elected dean emeritus.

IT is reported that Professor B. Brouwer has declined the invitation to organize a neurologic clinic at the Johns Hopkins University.

DR. ALEXANDER WETMORE, assistant secretary of the Smithsonian Institution, was elected president of the American Ornithologists' Union at its forty-fourth annual meeting at Ottawa, on October 11.

DR. GEORGE SHANNON FORBES, professor of chemistry at Harvard University, has been elected chairman of the division of physical and inorganic chemistry of the American Chemical Society.

PROFESSOR H. L. RIETZ, of the University of Iowa, has been appointed actuary of the Pension Commission of Chicago for the purpose of investigating the pension systems of the city.

DR. ALWIN BERGER, formerly associate horticulturist at the New York Agricultural Station and responsible for the botanical contributions to the recently published volume on "The Small Fruits of New York," has returned to Stuttgart, Germany, to head the department of botany of the State Museum.

PROFESSOR S. TERAO, of the Japanese Fishery Institute, will spend a year at the Rockefeller Institute in a study of the "growth and breeding of animals." He will leave Japan for America in October.

PROFESSOR F. WOOD JONES, of Adelaide, has accepted a Rockefeller foundation research appointment. He will go to the University of Hawaii to study racial problems in the Hawaiian Islands.

DR. BENGT HAMILTON, of Stockholm, Sweden, has accepted a position as head of the research laboratory of the children's medical department and associate in pediatrics at the Massachusetts General Hospital.

DR. S. D. WICKSELL, of the University of Lund, Sweden, has been appointed special lecturer at the University of Michigan, to be in residence during the second semester beginning in February, 1927.

PROFESSOR EJNAR HERTZSPRUNG, of the Observatory of the University of Leiden, will study variable stars for some months at the Harvard College Observatory. His visit to America is supported in part by the International Education Board.

DR. TOR JONSON, president of the Swedish Forest School at Stockholm, has been in this country since June and has visited a number of the forest experiment stations, arranging for cooperation between the American research institutions and the Swedish forest experiment station.

DR. W. H. LONGLEY, professor of biology at Goucher College, is spending a sabbatical year in Hawaii, Samoa and the Dutch East Indies, where he is continuing his researches upon tropical reef fishes under the auspices of the Carnegie Institution of Washington.

DR. EZRA ALLEN, professor of biology in Ursinus College, has been given a year's leave of absence in order that he may conduct researches in embryology under the auspices of the Carnegie Institution of Washington at Cold Spring Harbor, N. Y.

PROFESSOR F. H. HOWARD, of the department of physiology at Williams College, has been granted a leave of absence for the present academic year. He plans to work at the College of Physicians and Surgeons, New York.

DR. VERNON KELLOGG, secretary of the National Research Council, has returned to Washington after several months abroad.

MISS FRANCES DENSMORE, of the Bureau of American Ethnology, has returned to her home in Red Wing, Minnesota, after an absence of ten weeks in the field. During the latter portion of that time she recorded the songs of the Indians who were gathered in the hop fields at Chilliwack, British Columbia.

PROFESSOR HAROLD E. ROSS, of Cornell University, has returned from a year spent in Buenos Aires, during which time at the request of the Argentine government he gave his services as a bacteriologist in improving the quality of the milk supply.

DR. R. L. DOWDELL, assistant professor of metallography in the school of mines of the University of Minnesota, was engaged during the summer as research associate at the U. S. Bureau of Standards.

PROFESSOR WALTER S. HUNTER, G. Stanley Hall professor of genetic psychology at Clark University and editor of *Psychological Abstracts*, has sailed for Europe for a two months' stay. His purpose is to establish European contact for the new psychological journal and to secure assistants for the editorial board. During Dr. Hunter's absence his courses will be given by Professor J. W. Bridges, professor of psychology at McGill University.

DR. LOUIS E. WISE, professor of forest chemistry at Syracuse University, and Professor E. A. Hartley, of the department of forest entomology, are touring in Europe in the interest of forest research.

PRESIDENT FRANKLIN S. HARRIS, of Brigham Young University, Provo, Utah, is on leave during the present year traveling through the Orient and Europe. In Japan Dr. Harris gave a paper on the "Soil Alkali Problem of the Pacific Coast" before the Pan-Pacific Science Congress. During his absence L. John Nuttall, Jr., dean of the college of education, is acting president.

DR. MICHAEL I. PUPIN, professor of electromechanics at Columbia University, gave the principal address at the dedication of the new Henry M. Sanders laboratory of physics at Vassar College on October 18.

DR. A. J. CARLSON, professor of physiology at the University of Chicago, on September 28 spoke to the student body of the University of Colorado School of Medicine, Denver, on the importance of research work in the medical field. In the evening of the same day, Dr. Carlson delivered a lecture before the faculty of the medical school on the "Physiology of the Gastro-intestinal Tract." After the formal lecture a buffet supper was given in honor of Dr. Carlson.

DR. H. C. SHERMAN, Mitchill professor of chemistry at Columbia University, lectured on "Some Recent Advances in the Chemistry of Nutrition" before the Philadelphia section of the American Chemical Society on October 21.

E. B. CRAFT, vice-president of the Bell Telephone Laboratories, addressed a meeting of the New York Electrical Society on October 27, when he described and demonstrated the technical features of the "Vitaphone."

DR. COLIN G. FINK, of Columbia University, addressed a meeting of the Society of Automotive Engineers held at Detroit on the evening of October 21, when he spoke on "Chromium, a Metal of many Uses."

DR. GEORGE F. SWAIN, professor of civil engineering at Harvard University, will be one of the speakers at the fourth annual convention of the American Institution of Steel Construction to be held at White Sulphur Springs, W. Va., from October 26 to 30.

DR. ARCHIBALD YOUNG, professor of surgery at the University of Glasgow; Dr. David Wilkie, professor of surgery at the University of Edinburgh, and Dr. W. Sampson Handley, of London, visited the Mayo Foundation during the week of October 10 and were the speakers of the evening at the meeting of general staff on October 13. Dr. Young and Dr. Wilkie have come to this country to be the guests of the Interstate Postgraduate Assembly at the meeting in Cleveland, and Dr. Handley is one of the authorities on cancer from England who attended the International Cancer Symposium at Lake Mohonk, New York, in September.

PROFESSOR J. N. BRÖNSTED, of the University of Copenhagen, addressed the scientific staff of the Rockefeller Institute for Medical Research, on October 22, on "The Conception of Acids and Bases."

DR. J. SCHALTENBRAND, of Hamburg, Germany, addressed a meeting of the physiological conference at the Harvard Medical School on October 27, speaking on "Behavior of Thalamus Cats."

DR. RUDOLF ABEL, director of hygiene at the University of Jena, gave two Chadwick lectures of the Royal Society of Medicine in London on October 19 and 20 on "The Development and Present State of Public Health in Germany."

A COURSE of eight lectures on "The Interaction of Pure Scientific Research and Electrical Engineering

Practice" is being given by Professor J. A. Fleming, F.R.S., at the Institution of Electrical Engineers, London, on Wednesdays and Fridays in October and November, beginning on October 20.

A MEMORIAL was recently unveiled at Lebanon, Connecticut, to Dr. William Beaumont, the pioneer American physiologist, who studied gastric digestion through an accidental fistula on Alexis St. Martin.

A FRENCH medical conference which will take place from November 3 to 7 termed "Journées Médicales de Montpellier" will include the commemoration of the centenary of the discovery of bromine by Balard.

THE early home of David Livingstone at Blantyre on the Clyde is now for sale, and a movement has been started to purchase the house and grounds as a Scottish National Memorial to the explorer. In addition to the purchase of the house, it is planned to endow it as a museum for such Livingstone relics as can now be collected, as well as other exhibits illustrating the opening-up of Africa. A representative committee has appealed for £12,000.

DR. ROBERT G. REESE, professor of ophthalmology at Cornell University Medical College, died on October 18, aged sixty-one years.

DR. JOHN W. ADAMS, professor of surgery and obstetrics since 1896 in the Veterinary School of the University of Pennsylvania, died on October 23.

DR. JOHN W. BRODNAX, associate professor of anatomy at the University of Virginia Medical School, has died, aged sixty-two years.

DR. WILLIAM STURGIS BIGELOW, at one time vice-president of the Massachusetts Institute of Technology, died on October 6 at the age of seventy-six years.

THE American Society of Naturalists will hold its annual meeting in Philadelphia on Wednesday, December 29. The program, to be given in the afternoon of that day, will be a symposium on the general subject "Quantitative Biology." The dinner of the society will take place in the evening at the Hotel Walton. The presidential address to be delivered after the dinner by Professor J. Arthur Harris will be entitled "The Human Value of Quantitative Biology." No morning program will be offered.

THE thirty-fifth annual meeting of the American Psychological Association will be held at the University of Pennsylvania, Philadelphia, on Tuesday, Wednesday and Thursday, December 28 to 30, 1926, under the presidency of Professor Harvey A. Carr, of the University of Chicago. Scientific sessions will be held on the mornings and afternoons of each of these three days in the Laboratory of Psychology, College Hall. The annual business meeting will be held on the evening of Tuesday, December 28; the annual dinner and president's address will be held on Wednesday, December 29, at the Hotel Pennsylvania, the headquarters of the association. For information address the secretary, Professor Samuel W. Fernberger, College Hall, University of Pennsylvania.

THE fifteenth annual meeting of the American Association of Variable Star Observers was held at the Harvard College Observatory on October 23. The annual dinner was served at the Hotel Bellevue, with Dr. Ejnar Hertzsprung, of Leiden, Holland, as a guest and speaker. Other speakers included Dr. George R. Agassiz on "Meteor Crater," and former President D. B. Pickering, who told of his recent experiences abroad.

THE Spanish-American Aeronautic Congress opened on October 20 in Madrid. The program of the congress includes the adoption of terminology in Spanish and French; study and recommendation on international aerial legislation from the point of view of Spanish-American interest, with proposed modifications to the existing international conventions for aerial navigation, and international private aerial rights. It was planned that the congress would visit all the Spanish factories and flying fields during the four days of the congress. Don Juan Francisco de Cardenas, chief of the American Section in the Ministry of State, has been made president of the organizing committee.

ACCORDING to a radio message, the joint expedition of the New York Zoological Society and the Roosevelt Memorial Association which is attempting to retrace the steps of Roosevelt down the River of Doubt in Brazil have reached the upper Paraguay River near where the River of Doubt is supposed to rise. The headwaters of the Paraguay lie over a divide from the headwaters of the River of Doubt, which has been named the Rio Teodoro. The most difficult leg of the journey now is to begin, for the ridge is covered by a dense jungle.

THE U. S. National Museum has recently received as donations two important collections of insects. One of these is from John D. Sherman, Jr., of Mount Vernon, New York, comprising about 20,000 specimens of water beetles of the families Dytiscidae and Haliplidae, and representing about forty years of entomological activity on the part of the donor. This Mr. Sherman gives "in loving appreciation and honor of Dr. E. A. Schwarz." The second donation is from Dr. E. A. Chapin, formerly of the Bureau of Animal Industry, but recently appointed to the staff of the

Bureau of Entomology as a specialist in beetles. This collection comprises fleas, lice and other ectoparasites, and is the largest addition in this group ever received by the museum.

A FUND of $1,500 to establish a fellowship in chemical engineering for pulp and paper research has been accepted from the Northwest Paper Company of Cloquet, Minn., by the University of Wisconsin. George Brabender, of Peshtigo, a graduate of the University of Wisconsin with the class of 1925, will conduct the work under the direction of Professor O. L. Kowalke, of the department of chemical engineering. In tendering the fellowship the Northwest Paper Company indicated its intention to continue it indefinitely if the preliminary work proves of sufficient merit.

MORE than 900 specimens of Siamese birds have been added to the collection at the United States National Museum through a large shipment of skins received from Dr. Hugh M. Smith, who is now in Siam. Dr. Smith will remain there for some time investigating the fish fauna for the Siamese government. During that time he also plans to send further collections to the United States.

AN orchid garden containing 7,000 plants and representing many species of the tropical flower has been taken over by the Missouri Botanical Garden from C. W. Powell, of Balboa. Mr. Powell has made a special study of the orchids of Panama and has more than doubled the number of species formerly known from that country, besides discovering more than a hundred new species. The orchid garden will be maintained in the Canal Zone as an experimental tropical garden.

ACCORDING to a special cable to the New York *Times* annual public lectures on health and disease for the benefit of the public will be conducted, beginning next year, by the British Medical Association in response to pressure from many sources. This decision of the council of the association follows the recent address before the general body by Sir Thomas Horder, in which he declared that the public had decided it wanted lectures on health and meant to have them. The association has in the past frowned on popular medical lectures, on the ground that they were unethical.

UNIVERSITY AND EDUCATIONAL NOTES

A GIFT of $3,385,000 has been made to the University of Chicago by the General Education Board for the medical school. The gift is contingent upon the obtaining by the university of an additional $2,000,000.

BY the will of the late Mrs. Gertrude Baldwin Woods, of Cambridge, Mass., Harvard University will receive $20,000 and Oberlin College $25,000.

AT the Harvard Medical School, Dr. James B. Ayer has been named clinical professor of neurology and will take the place of Dr. Edward W. Taylor, who has resigned.

DR. H. R. KRAYBILL, formerly bio-chemist at the Boyce Thompson Institute for Plant Research, has been appointed professor of agricultural chemistry in Purdue University and state chemist and seed commissioner.

PROFESSOR H. S. UHLER has resigned as head of the department of physics at Gettysburg College to become associate professor of physics in the Sheffield Scientific School of Yale University.

DR. SAMUEL STUART MACKEOWN, of the research department of the Westinghouse Electric and Manufacturing Company in New York, has been appointed assistant professor of electrical engineering at the California Institute of Technology, Pasadena.

DR. L. R. VAN WERT, of the Harvard Engineering School, has been appointed assistant professor of metallurgy in the college of engineering at the Carnegie Institute of Technology.

DR. NATHAN L. DRAKE has recently been appointed professor of industrial chemistry at the University of Maryland.

DR. N. M. GRIER has been appointed head of the department of biology at Des Moines University, Iowa.

DR. HARVEY ALFRED ZINSZER, formerly instructor of physics at Lehigh University, has been appointed acting professor of physics in Mississippi State College for Women at Columbus.

JOSEPH C. CHAMBERLIN, until recently attached to the department of entomology of the University of California Citrus Experiment Station, has accepted a teaching position in the department of biology of the San José State Teachers College.

THE council of the University of Leeds have appointed E. L. E. Wheatcroft, M.A. (Cambridge), to the newly-created chair of electrical engineering.

DISCUSSION AND CORRESPONDENCE

AN OCCIDENTAL BUDDHIST'S CONCEPTION OF PERSONALITY

THE following quotation from Lafcadio Hearn[1] seems of considerable interest, especially in view of

[1] "Gleanings from Buddha Fields," pp. 92–94.

the fact that he probably knew absolutely nothing of Mendelian inheritance. It recalls delightfully Professor Jennings's address years ago in Princeton as president of the American Society of Naturalists.

Whether you (by *you* I mean any other agglomeration of souls) really wish for immortality as an agglomeration, I cannot tell. But I confess that "my mind to me a kingdom is"—not! Rather it is a fantastical republic, daily troubled by more revolutions than ever occurred in South America; and the nominal government, supposed to be rational, declares that an eternity of such anarchy is not desirable. I have souls wanting to soar in air, and souls wanting to swim in water (seawater, I think), and souls wanting to live in woods or on mountain tops. I have souls longing for the tumult of great cities, and souls longing to dwell in tropical solitude;—souls, also, in various stages of naked savagery;—souls demanding nomad freedom without tribute;—souls conservative, delicate, loyal to empire and to feudal tradition, and souls that are Nihilists, deserving Siberia;—sleepless souls, hating inaction, and hermit souls, dwelling in such meditative isolation that only at intervals of years can I feel them moving about;—souls that have faith in fetiches;—polytheistic souls;—souls proclaiming Islam;—souls mediaeval, loving cloister shadow and incense and glimmer of tapers and the awful altitude of Gothic glooms. Cooperation among all these is not to be thought of: always there is trouble,—revolt, confusion, civil war. The majority detest this state of things: multitudes would gladly emigrate. And the wiser minority feel that they need never hope for better conditions until after the total demolition of the existing social structure.

I an individual,—an individual soul! Nay, I am a population,—a population unthinkable for multitude, even by groups of a thousand millions! Generations of generations I am, aeons of aeons! Countless times the concourse now making me has been scattered, and mixed with other scatterings. Of what concern, then, the next disintegration? Perhaps, after trillions of ages of burning in different dynasties of suns, the very best of me may come together again.

MAYNARD M. METCALF
THE JOHNS HOPKINS UNIVERSITY

PROFESSOR OSBORN ON THE MAMMALS AND THE BIRDS OF THE CALIFORNIA TAR POOLS

IN the magazine *Natural History,* Volume XXV, pages 527–543, Professor H. F. Osborn has a paper on certain mural paintings in the American Museum of Natural History. These depict the trapping of Pleistocene mammals and birds in the asphalt pits near Los Angeles. Professor Osborn places the scenery and the existence and activities of the La Brea animals in the Ice Age. The mountains were there as now, but their summits were covered with snow, and this snow reached much farther down the mountainsides than it does now, even in winter time. He has, therefore, had his artists represent the higher peaks as heavily mantled in white.

Professor Osborn proposes a new theory to account for the presence of so many large animals in the tar pits. Instinctively the creatures avoided the dangerous pools, except perhaps during seasons of drought. Then they risked their lives to quench their thirst with the water thinly covering the treacherous tar. Would that the author of the theory had presented some evidence to show that, during that boreal climate and in that topographical situation, there was any such dearth of water. Even in the heat of summer at least a few streamlets probably descended into that rancho from the Santa Monica Mountains. Surely around the borders of the snow drifts of Old Baldy and of Mount Wilson, both plainly in sight, the thirsty animals might have found safe footing and water refreshing and abundant.

Professor Osborn suggests that intelligence, or the lack of it, played their part in the catastrophe. Three times as many camels and bisons were caught in the tar as of horses. Inasmuch as seventeen horses were entrapped and only thirteen Shasta sloths, the latter must have been rather superior beasts; but how can we measure the stupidity of the two thousand tigers and the three thousand wolves which perished there?

The earliest division of the Pleistocene epoch was one of the glacial stages. Ralph Arnold tells us (Mem. Calif. Acad. Sci., vol. III, p. 66) that at this time the Lower San Pedro beds were deposited and that they are filled with the shells of cold water mollusks. They repose on late Pliocene beds which also contain boreal mollusks. We have, therefore, evidence within a few miles of Rancho La Brea that the Pleistocene opened with a stage of cold climate.

Therefore, were there at that time and place any such animals as Professor Osborn is dealing with? Many of the mammals and of the birds were immigrants from South America; the native species were not of boreal, but warm temperate habit. It is evident, therefore, that Professor Osborn has referred his fauna to the wrong stage and the wrong kind of stage.

Immediately after the Lower San Pedro cold stage the Upper San Pedro beds were laid down; and these indicate a warm climate. As Arnold says (*op. cit.,* p. 29), the marine fauna living at San Pedro more nearly resembled that now existing on the coast two or three hundred miles farther south. In these beds, at San Pedro, species of mammals have been found which occur at La Brea, among them *Elephas*

imperator. It can not, in the writer's opinion, be successfully controverted that the La Brea fauna existed during the first interglacial stage.

Scenes like that represented by the mural doubtless occurred, but they must have been infrequent. Osborn's census (his page 528) shows 5,237 animals caught. If the time required was two thousand years the risks run by elephants, camels, sloths and horses were not great. An elephant and a horse were entrapped in a century; a sloth and a camel every thirty-three years; a bison a little more frequently. The saber-tooths and wolves depended little for their living on the catches of those traps. Twenty-one carnivores perished for each herbivore. Evidently there is some as yet unthought-of explanation for the destruction of the wolves and saber-tooths. Professor Osborn's theory of illusory waters in a thirsty land is inadequate. Horses, bisons and elephants require water even more than do wolves and tigers.

Probably there are those who believe that the history of the tar pits was a short one and that the sanguinary performances enacted there were more nearly continuous; others, as the writer, that the history was much longer than two thousand years.

OLIVER P. HAY

WASHINGTON, D. C.

AGE OF PRESBYOPIC VISION AS AN INDEX OF THE LONGEVITY OF PRIMITIVE MAN

IN normal individuals the onset of presbyopia during the fifth decade of life is a very striking and constant phenomenon. The loss to the individual of the ability to adjust his crystalline lens for near vision can not be regarded as otherwise than a handicap to that individual. Even in the life of primitive races such loss must of necessity have placed its victims in an unfavorable condition as compared with younger members who possessed clear vision from a foot to a hundred feet. In the case of modern civilized man such loss is very obvious and would work serious hardships were it not an easy matter to correct it by means of appropriate lenses of glass. The point I wish to bring out is that if primitive man had lived in any numbers beyond the fifth decade, it is not improbable that there would have developed through the survivorship of the more fit a race which would have retained elasticity of the crystalline lens for more decades than five. It is well appreciated that the above idea may be a case of "the cart before the horse" and that presbyopia may have been a small one of several factors causing the members of primitive races to die at relatively earlier ages as compared with the average age of death of modern civilized man. That a small percentage of persons possess an elastic lens for more than five decades shows the possibility of postponing presbyopia had there been any occasion for applying the law of natural selection. The presbyopic age falls in so closely with the average age of longevity in the past as expressed by Dr. T. Wingate Todd in "Age: the Piper," SCIENCE, September 3, 1926, that the matter seems to be more than a coincidence.

M. W. LYON, JR.

SOUTH BEND, INDIANA

THE INDICATION OF QUOTATIONS

WHEN the reader of a scientific paper encounters quotation marks he usually apprises his audience of the fact in one of various ways; he may say "quotation" where it begins and "end of quotation" at the end, or he may say "quotes" and "quotes closed." Where quotations are frequent the reader may fail to indicate them accurately because of the time and awkwardness involved, and misunderstanding may result. This difficulty has arisen, also, in the reading of proof by one person to another.

Perhaps there is a reader of SCIENCE who knows of something that has been done toward simplifying the indication? It is suggested that matters might be improved by using more easily vocalized expressions and by taking advantage of the inflection of the voice. The expression "coo," for example, might be conventionalized as the indicator of the beginning of a quotation and "cō̄" as the indicator of the end. These combinations are quickly and easily vocalized and the falling inflection in "cō," which occurs naturally, suggests the end of the quotation. Indicators of this character would scarcely be confused with the content of the paper being read.

Where conditions permit, perhaps tapping would afford a more effective mode of signalling. Two taps of a pencil on the table, for example, might be made the pre-quotational and one tap the post-quotational signal. Such indication would obviously stand out from the context.

One lecturer has been known to signal quotation by an arm gesture symbolic of brackets. A disadvantage of inaudible gesture, though, is that it is not noticed by those members of the audience who do not happen to be spectators at the right moment.

S. M. NEWHALL

YALE UNIVERSITY

THE CALORIMETRIC METHOD OF DETERMINING BLOOD FLOW IN THE EXTREMITIES

DR. STEWART'S communication, which appeared in SCIENCE for September 3, 1926, and which carries the title quoted above, is presumably a rejoinder to my special article which was printed in SCIENCE July 2, 1926, under the caption: "On the Elimination of Heat

from Normal and Pathologic Subjects as Determined from Calorimetric Studies of the Extremities." It is possible that Dr. Stewart did not appreciate the fact that my article in SCIENCE is, in large part, only an abbreviated report of my conclusions and does not contain the data which I believe support them. The complete paper, together with the separate contributions on this general topic by two of my colleagues, will appear shortly in the *Journal of Clinical Investigation*. It does not seem to me either necessary or desirable to make any further comment, inasmuch as I should be rehearsing selected and hence disconnected portions of the data and discussion given in the complete paper. I am prepared to rest my case upon the materials contained in the full text of my experiments and conclusions.

CHARLES SHEARD
MAYO CLINIC AND MAYO FOUNDATION,
ROCHESTER, MINNESOTA

SCIENTIFIC BOOKS

The Growth of Biology. Zoology from Aristotle to Cuvier, Botany from Theophrastus to Hofmeister, Physiology from Harvey to Claude Bernard. By the late WILLIAM A. LOCY. New York, Henry Holt and Company, 1925.

HAVING used Locy's "Biology and Its Makers" (Third Edition, 1915) as a text-book for a number of years, I have, in common with many others, a sense of deep obligation to its author. The historical method of treatment has many advantages, not the least being that the human story always arouses and holds the interest of students. Locy succeeded in writing a book which was at once sound, reasonably comprehensive and attractive. It was no small task, especially as the greater part necessarily dealt with the labors of Europeans, and many desirable works of reference were not easily obtained. It was only too possible to be misled by imperfect or erroneous information, or to fail to preserve a balance between the various subjects and men according to their importance. Yet the book has stood the test of use very well, and among current text-books easily ranks as one of the best. One obvious criticism related to the title, which led us to expect a history of biology, whereas botany was practically ignored. This was inconvenient, because it is common to treat the history of biology as a whole, and the teacher of such a course, using this text, had to prepare supplementary matter dealing with the botanical side. Even so, botany could not be adequately presented, and the course was to that extent lopsided. Evidently the author came to feel that this was true, so he set to work to write another book, which should actually cover the whole biological field. Unfortunately, this was not finished at the time of his death in October, 1924, but there was enough to make a volume, about equal in bulk to the earlier text. This is now before us, and bears the title "The Growth of Biology." It fully maintains the author's previous standard, and while there are many passages borrowed from the other book, there is very much that is new and extremely interesting. Very appropriately, we begin with a discussion of those early biological studies represented by the wonderful work of paleolithic man. While the mammoth was still alive, there were zoological artists (as we should now call them) to picture the great beast with skill sufficient to indicate its disposition as well as its form. Although Locy does not say so, it is evident that the ancient cave man discovered the principle of the moving picture. The running herd of reindeer is shown with the horns of the hindmost animals greatly elongated, indicating speed. The infuriated mammoth appears to have several trunks, showing how he waved his trunk in the air when enraged. Between this period and that of Aristotle was a very long story of human culture, and, as Locy remarks, no wonder the Greek philosopher spoke of the "ancients." However, it is pointed out that before the time of the Greeks "science was an anonymous social product," growing by slow degrees as observations were added to current tradition. It is still in that stage among uncivilized tribes. But certain individual Greeks made it their business to collect and arrange scientific data, and thus we have the beginnings of organized zoology, botany and physiology. Full justice is done to the versatility and learning of Aristotle, passages of whose works sound extraordinarily modern even to-day. Theophrastus, as the father of botany, is dealt with at some length. Then we pass up through the centuries, and there is a good account of the early printed books on natural history, with copies of the often crude illustrations. Brunfels's figure of the plantain (p. 129) is upside down. The fabulous "Su," figured on p. 296, appears to have been based on some one's recollection of a woolly opossum (Caluromys). The early microscopes and the discovery of micro-organisms are discussed, with copies of the old figures. It has been suggested that Roesel's "kleine proteus" was not the amoeba, but Pelomyxa, the latter being more recognizable on account of its larger size. But Roesel's original figure, copied by Locy, is evidently that of the amoeba. We are also shown Leeuwenhoek's original illustrations of bacteria; how little could he realize the tremendous importance of the group of organisms he had discovered! There is a most interesting chapter, "The Period of Hofmeister; with a Digression on Text-books and Improvements in

University Teaching." Evolution, genetics and the later history of bacteriology and protozoology are among the important subjects which unhappily failed to receive treatment. Consequently, the teacher is puzzled to know which book to use. He can not very well require students to purchase both. The omissions from the newer book are so serious that it seems to me they are not balanced by the gains. What we now need is a combination of the two books, with such condensation as may be necessary to keep the size down to reasonable limits. Certain modern researches should if possible be described, if only to show the fruition of earlier investigations. Aside from use as college texts, both books are well adapted for general reading, and should be in all public libraries. They can also be used to great advantage as reference books in high schools, on account of the many and excellent illustrations, which clarify the moderate technicalities of the text.

Personally, I do not like the method of splitting up the work of authors according to subjects, so that, for example, Leeuwenhoek is dealt with on pages 206 to 218, and again on pages 250 to 253. To overcome the confusion in historical perspective I have, in using the earlier book, prepared chronological tables by means of which the student may see what were the principal events in the whole field of biology at any particular time. Tables of this sort would form a useful appendix to the book. However, anything which may be said in criticism is of very small moment compared with the great services which Locy has rendered to the history of biology, and thereby to education.

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SCIENTIFIC APPARATUS AND LABORATORY METHODS

VACUUM TUBES FOR THE STORAGE AND SHIPMENT OF BACTERIA[1]

IN December, 1924, a progress report was made on "The Preservation of Bacteria in Vacuo."[2] The method described and still in use for the preservation of most of our stock cultures consists in drying the cultures on small bits of filter paper placed in small cotton stoppered tubes within pint milk bottles with specially ground covers. After establishing a fairly high vacuum the bottles are kept in the refrigerator. Under these conditions most bacteria remain alive for several years.

[1] From the Department of Pathology and Bacteriology of Johns Hopkins University, Baltimore, Maryland.

[2] Brown, J. Howard, "The Preservation of Bacteria in Vacuo." *Abstracts of Bacteriology*, Jan., 1925, ix, 8.

For certain purposes it has seemed desirable to preserve cultures in individual vacuum tubes rather than in pint bottles, especially for shipment by mail.

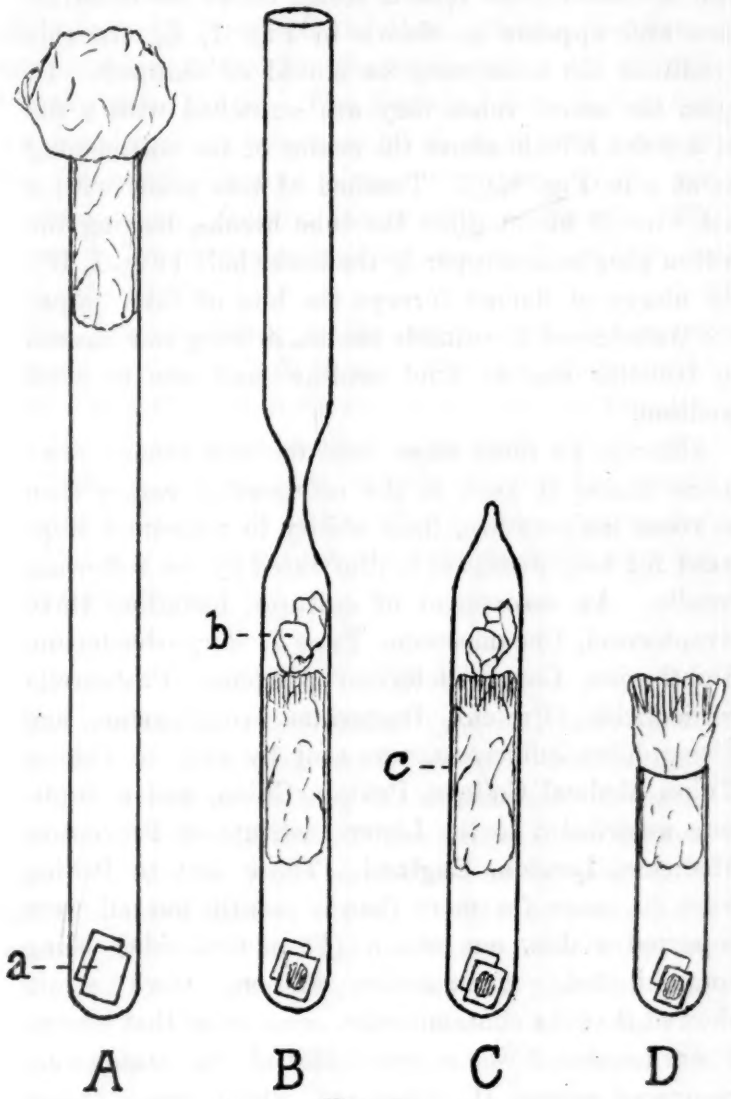

FIG. 1

For this purpose small test tubes (Fig. 1, A) about seven mm in diameter and nine cm long are employed. The tubes contain two small bits of filter paper (a), are plugged with cotton wool and sterilized by dry heat. Broth cultures are mixed with an equal volume of blood or serum. Agar slants should have two or three drops of blood or serum added to the condensation fluid. A loopful of the mixture is placed on each of the bits of filter paper within the tube. The cotton plug is then cut off with scissors just above the mouth of the tube; the plugged end of the tube thoroughly flamed and with a rod or pair of forceps the plug is pushed down into the lower half of the tube just above the filter papers. Two or three lumps of calcium chloride (previously sterilized by dry heat) are placed above the cotton plug, as shown at b in Fig. 1, B. In the small flame of a blast lamp or of a micro-burner a constriction is made at about the center of the tube, care being taken not to obliterate the lumen (Fig. 1, B). The tube is now to be connected

by means of pressure tubing to the vacuum pump,[3] which should have a manometer connected with it. When the air pressure has been reduced to 20 or 30 mm of mercury the tube is sealed off at the constriction and appears as shown in Fig. 1, C. In this condition the tubes may be stored or shipped. To open the sealed tubes they are scratched with a file at a point a little above the center of the cotton plug as at c in Fig. 1, C. Touched at this point with a hot wire or bit of glass the tube breaks, leaving the cotton plug as a stopper in the lower half (Fig. 1, D). By means of flamed forceps the bits of filter paper are transferred to suitable media, it being our custom to transfer one to fluid medium and one to solid medium.

Although in these tubes most bacteria remain alive much longer if kept in the refrigerator rather than at room temperature, their ability to withstand shipment for long distances is illustrated by the following results. An assortment of cultures, including three streptococci, Pneumococcus Type I, Corynebacterium diphtheriae, Corynebacterium pyogenes, Pasteurella cuniculicida, Proteus, Bacterium sanguinarium and Hemophilus influenzae, was sent by mail to Peking Union Medical College, Peking, China, and a duplicate assortment to the Lister Institute of Preventive Medicine, London, England. Those sent to Peking were en route for more than a month, but all were reported viable, one strain (P. cuniculicida) being contaminated. Examination of our stock strain showed that the contamination came from that source. From London it was reported that all the strains were recovered except H. influenzae, which was contaminated. A culture of Meningococcus later sent to London did not survive. We are indebted to Dr. Carl TenBroeck, of Peking Union Medical College, and to Drs. J. C. G. Ledingham and C. J. Martin, of the Lister Institute, for reporting on the viability of these cultures. Numerous cultures sent to various parts of the United States have been received in viable condition.

The method of putting up cultures in individual vacuum tubes for shipment has the following advantages: Saving in bulk and weight for mailing. Less danger of breakage or, if broken, less danger of contaminating the packing. A large number of vacuum tube specimens can be prepared from a single tube of growing culture. Cultures received in vacuum tubes need not be transplanted immediately, but may be stored for some time. A longer period of viability for most bacteria.

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[3] A satisfactory pump for this purpose is the Hyvac Pump of the Central Scientific Company, of Chicago, Ill.

SPECIAL ARTICLES

UNDEFORMED PREHISTORIC SKULLS FROM THE SOUTHWEST[1]

INTRODUCTION

THE work of the archeologists in the American southwest is more and more becoming truly scientific and being conducted in a systematic manner, at least in a few districts. Hence we may look forward to satisfactory answers perhaps soon forthcoming in regard to culture characteristics, sequence, relations and origins.

But the task of the physical anthropologist is lagging far behind. The work has been sporadic, unsystematic and, as a result, we know little about the ethnic characters, relations and possible origins of the prehistoric populations of the southwest.

Archeology recognizes now two main cultures; the older is that of the so-called Basket Makers, the more recent belongs to the Pueblo Cliff Dwellers. The latter were brachycephalic people with heads artificially deformed posteriorly. The former had undeformed long skulls. But until now practically nothing has been published concerning them. In the past year I studied and measured all such crania as I could obtain. This is a very short résumé of my work in this line.

MATERIAL STUDIED

The following constitutes the basis of my study and conclusions:

(1) From Colorado: one female from Piedra (Archuleta Co.); one female skull from La Boca (La Plata Co); four female crania, more or less complete, and one fragment from La Plata Valley.

(2) From New Mexico: one female from near Rosa (Rio Arriba Co.) and fragments.

(3) From Arizona: three males and one female from Cañon del Muerto. Besides these, which I measured personally, I obtained, through the kindness of Dr. Hooton, of Harvard, pictures and measurements of four males and three females from the Marsh Pass district of Arizona. Also the principal measurements and indices of seventeen males and three females from Coahuila, Old Mexico; and finally 178 males and 137 females from Santa Barbara Islands, California, previously measured by Lucien Carr.

Dr. Hooton himself recognized the similarity existing between the Arizona skulls he studied and the Coahuila crania as well as the California skulls. I

[1] Address read before Section H—Anthropology—American Association for the Advancement of Science, Kansas City, December, 1925.

extended the resemblance to all those I measured. I have thus a sufficiently large series of diversified origin to be able to draw some conclusions as to general type and later, perhaps, to establish the local variations or diversities of the main type.

GENERAL CHARACTERS OF THE SKULLS OF BASKET-MAKER TYPE

In norma verticalis the skull is generally ovoid and tending to a pentagonal shape with often a narrow, pointed and protuberant occipital. The most dolichocephalic are the females from La Plata, with 71.62 as horizontal index. The average for both sexes of the southwest is 75.72 and for Coahuila 75.11. The Santa Barbara skulls are slightly broader, with 76.73.

The most hypsicephalic crania are again the La Plata females, averaging 107.29, as height-breadth index. The others from Colorado are also above 100, the average for the whole southwest being 98.52 and for the Coahuila series 98.48. The Santa Barbara crania are lower, 94.59. Many of the southwest skulls are clearly scaphoid.

In norma occipitalis most of these skulls present a pentagonal shape with prominent parietal tuberosities, especially in females, and the parietals vertical or nearly so. The occipital crest and inion are often well developed.

In norma frontalis they display a forehead many times narrow, especially in females, somewhat low and slanting, with frontal bosses close together and not very visible. The nasal depression is seldom well marked. The superciliary arches are at times fairly prominent, at least on the first third from the glabella. The orbits are large and squarish as can be seen by the orbital index of Colorado females from 90.29 to 94.45 and for the Sta Barbara series averaging 92.39, truly megaseme, while the average for the Arizona index is 85 and 86.43 for Coahuila, thus showing a difference between the two groups. The nasal index is also interesting with the hyperplatyrrhine skulls from Piedra, 65.16, and a fragment from New Mexico, 65.21. The others from Colorado and New Mexico are also platyrrhine, 53.7 to 56; those from Arizona a little less so, 51.47 to 52.92; the Coahuila crania with 49.68 and Sta Barbara 48.39, being mesorrhine. The nasal aperture is broader and bridge lower in females. The lower borders are usually smooth and even indistinct.

The total facial index of only eight skulls from the southwest could be measured; it averages 85.79, near the lower limit of mesoprosopy. The upper facial index is known for a much larger number. It varies from 48.49 for females from Canon del Muerto and 48.51 from Piedra, Chamæprosope, to an average of 52.17 for the whole southwest, 52.45 for Sta Barbara and 55.02 for Coahuila; the females with shorter faces than the males in all series.

The palate is in all the specimens I have seen U-shaped, short and broad, the extreme cases being those from La Boca 128 and Rosa 128.84. The average maxillary index for the southwest is 120.48 and for Coahuila 119.69. The gnathic index, higher for the females of all series, is 97.36 as general average for the southwest and almost the same, 97.10, for Coahuila and slightly larger for California, 99. There is alveolar prognathism especially visible in Colorado skulls.

The largest skulls are the males from Canon del Muerto, averaging 1,503 ccm. For the whole southwest I found 1,410 for males and 1,255 for females, the Sta Barbara crania being smaller, respectively, 1,372 and 1,248, and the Coahuila showing less sexual difference, 1,393 and 1,338. The general average for 200 males is 1,374 and for 150 females 1,250, showing then a small capacity.

DISTRIBUTION OF TYPE

As already seen this cranial type has been found in southwest Colorado, northwest New Mexico and northern Arizona. I have no doubt that the skeletons discovered with archeological remains of Basket-Maker culture in Utah belong to this type, as their artifacts, according to Dr. A. V. Kidder, are of the same kind as and probably contemporary with the culture of the Arizona sites. Then this physical type would have occupied the whole San Juan basin and territory west of the Colorado River for some two thousand years B. C. The Coahuila remains constitute the Mexican district and the Californian district is represented by the large series from Santa Barbara Islands and opposite coast as well as the Pericue of Lower California reported by Ten Kate. A northeastern group would be constituted by many prehistoric skulls of the same type belonging to Algonkian, Iroquois and Erie Indians, and some mounds of the Ohio and Tennessee, according to Dr. R. Dixon. But I have no direct knowledge of these crania.

In South America, according to the anthropological literature in various languages, we see three groups. The western group in Ecuador, Paltacalo (Dr. Rivet), Punin calvarium (Sullivan and Hellman), and in Colombia (Dr. Verneau). The southern group, islands and southern coast of Chile, Tierra del Fuego, and Tehuelche of Patagonia (Dr. Verneau). The eastern group in Brazil, coast and highland, the Lagoa Santa race.

All have many common characteristics. Their variations may be due to local adaptation or crossing with neighboring tribes.

INTERPRETATION OF DISTRIBUTION

All these prehistoric remains have been found either along marginal zones (coasts or islands) or in places of refuge (islands, rocky coast, deserts or very cold climate). This seems an evidence of antiquity and of having been pushed aside and away as by a central drive from newer and stronger tribes splitting an early population into small groups, isolating them, reducing them to live in less favorable districts and at the periphery of the country.

POSSIBLE KINSHIP WITH POPULATIONS OUTSIDE OF AMERICA

After a comparison of cranial measurements, proportions and characteristics of skulls, it seems more and more acceptable to believe that this type of prehistoric Indians is resembling, and even is possibly related to, populations scattered in Oceania between Australia and New Caledonia in the southwest, New Guinea and New Britain in the northwest and as far east as Hawaii and Easter Island. Ten Kate, Rivet, Sullivan and other anthropologists speak of Polynesians and Melanesians; Verneau and a few others favor the Papuans.

Recent investigations by Dr. Rivet, of Paris, seem to establish a relation between Australian and Polynesian languages on the one hand and on the other the Tson dialect of the Tehuelche of Patagonia and the Hoka of California. Linguistic and anthropological considerations appear then to point out in the same direction.

The center of origin and dispersion of these people, their respective routes of migration, the time of their arrival in America and other highly interesting problems must remain unanswered questions, at least for the present.

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THE LOSSES IN TROUT FRY AFTER DISTRIBUTION

THE losses in trout fry which were found by the Biological Board of Canada for three months of each of the summers of 1923, 1924 and 1925 have ranged from 73 per cent. to 100 per cent.

Probably all fish culturists will agree that the two most effective means of reducing losses are (1) to protect the fry from enemies and (2) to provide them with an adequate supply of food. These are fundamental conditions for avoiding loss, whether in the hatcheries, in the retaining ponds or after distribution.

It does not seem to be a wise policy to protect and feed fry in hatcheries and in rearing ponds and then distribute them in wild ponds and streams where they have no protection from enemies and in many cases an inadequate food supply.

If seining or other forms of netting wild lakes and streams are carefully carried out, then both protection and food will be assured and losses will be greatly reduced. This was well shown by the Biological Board of Canada in the summer of 1923. One brook was partially seined of enemy fish and five thousand fry distributed in it. A second stream similar to the first was left unseined and five thousand deposited in it. At the end of three months both streams were seined and the survivors counted, with the result that fifteen hundred or 30 per cent. were alive in the former and only 175 or 2 per cent. in the latter.

Another example has been communicated to me by Mr. Albert French, vice-president of the Mastigouche Fish and Game Club, of New York City. The fishing areas of this club are about one hundred miles northeast of Montreal. Mr. French sent me the following letter after reading my article in SCIENCE of December 25, 1925.

> The failure of our trout to breed satisfactorily has been due, we are told by the authorities in Washington, to the fact that our lakes were out of "balance," that is to say, the suckers were in the ascendant, and we therefore started a warfare upon them in aggressive manner. At the mouth of the various brooks leading into our lakes we placed fyke nets in the early spring, as the suckers have a habit of going up stream (practically as soon as the ice is out) for spawning purposes. They went into these nets and were destroyed by men employed for this purpose.
>
> In addition to these fyke nets, I had built a large number of wire traps (which were made out of one half inch mesh galvanized wire) the size of a flour barrel, with one end flat, the other conical shaped, allowing convenient ingress for a sucker weighing two pounds. These traps were baited with stale bread and were placed at various points in our lakes, with a rope and float attached, and every second day were emptied by the man in charge. Carefully kept records show that up to the present time we have destroyed 767,000 suckers, and in the instance of at least one of our lakes, we feel that we have now gotten it in "balance."

The Mastigouche Club runs its own hatchery and Mr. French tells me that all the food which is required to feed both fry and fingerlings while in the hatchery or retaining ponds is obtained from the enemy fish. For a change of diet they are occasionally fed on liver and heart.

The expense of this investigation was borne by the Biological Board of Canada, and the work was done under the supervision of the chairman, Dr. A. P. Knight.

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